

Peer-review report of

Selvanathan, H.P., Hornsey, M. J., Jetten, J., Bourdaniotis, X., Hewett, M. & Manoharan, R.T. (2026). Understanding public responses to counter-protests disrupting social change movements. *advances.in/psychology*, 1, e648056.
<https://doi.org/10.56296/aip00049>

Round 1

Editor Decision

Dear Authors,

Thank you for submitting your manuscript, “Understanding the Effect of Counter-Protests on Public Attitudes towards Social Change Movements,” to our special issue on the Psychology of Pushback. We were fortunate to receive reviews from two experts with deep expertise in this area, and we are grateful for their constructive and focused feedback. We also conducted our own careful evaluation of your work before considering the reviews.

All of us recognize the clear strengths of your submission. Both reviewers emphasize its originality and social significance. Reviewer 1 observes, “The paper tackles an important question, namely, how mass publics react to pro-system counter-protests. This is a pressing topic in need of further psychological clarity, and this paper meets this need.” Reviewer 2 similarly remarks, “I read the manuscript with great interest. Focusing on counter-protests as active agents in shaping public opinion is both timely and underexplored.” Both reviewers also commend the inclusion of multiple cultural contexts. We concur with these positive assessments. Your work has valuable theoretical and practical implications. The manuscript is also well written, and the studies build coherently on one another.

At the same time, both reviewers identified limitations that warrant your attention, and we invite you to submit a revised manuscript that addresses these issues. As you will see from their reports, the reviewers devoted considerable time and effort to providing thoughtful feedback and constructive recommendations. While we ask that you respond to each of their points in your revision, we will not reiterate them here, as they are already clearly articulated. Instead, we wish to highlight a few issues we consider particularly important, in addition to concerns we identified ourselves.

Reviewer 2 raises questions about the broader framing of counter-protests as repression. As they note, counter-protests can often serve quite the opposite function. We encourage you to elaborate on this in both the introduction and the general discussion, including consideration of the generalizability of your results beyond the specific asymmetries studied here. Relatedly, both reviewers, while acknowledging the cultural contexts you studied, call for further reflection on how the findings might extend to other, potentially less democratic, contexts. A useful way to

address these points may be through a dedicated “Constraints on Generality” section in the discussion that specifies, based on a well-developed rationale relating to the current work and the broader literature, possible boundary conditions.

Like both reviewers, we believe that the study discussion sections and the General Discussion should be elaborated to identify and explain ways of addressing limitations of the current work. Currently, those sections of the manuscript mainly describe the results obtained; we ask that you provide a more critical analysis of the strengths and limitations of your work, noting inconsistencies in your data, offering interpretations (guided by the relevant literature), and explaining ways to test these interpretations. All research has some “loose ends,” and it is important that you alert readers to these and consider these issues in stimulating and productive ways.

There are, for example, limitations in your design and analyses for determining causality, not just in your survey studies but also with respect to mediation in your experiments. Much of your analysis relies on fully correlational or semi-correlational mediation. Because the association between mediator and outcome is inherently correlational throughout, you should explicitly consider reverse causality and discuss indirect effects with greater caution, particularly in the absence of main effects. For instance, please revise this sentence in the abstract: “This in turn predicted [replace with ‘was associated with’] greater sympathy towards protests for social change.” Similarly, the concluding portion of statements such as the following seems unwarranted given the lack of causal effects on the dependent variables: “When a peaceful social change protest is met with violent opposition, observers appear to view this as an illegitimate silencing tactic, which bolsters support for the initial protesters.” One reviewer also suggests possible statistical strategies worth pursuing, which we encourage you to consider. However, we note that these will not fully resolve the underlying causality issue. As an important future direction, you can thus discuss the value of additional studies that manipulate the mediator directly (see Spencer, Zanna, & Fong, 2005).

Relatedly, you attribute some inconsistencies in the main effects on the dependent variables to the possibility of suppressor or opposing effects. Another, perhaps more plausible, explanation that warrants discussion is that there may simply be little direct effect on how movements themselves are evaluated, with the stronger impact instead lying in perceptions of rights violations. We encourage you to unpack this more thoroughly in the discussion.

As Reviewer 2 notes, the role of political orientation also merits closer examination. While you acknowledge the possibility of moderation by political orientation and suggest it as an avenue for future research, your current studies are well positioned to address this directly. Including such moderation tests would substantially strengthen the contribution of the paper.

In the General Discussion we also ask that you reflect upon the meaning of your mediator, perceived suppression of free speech. For example, are the effects you report restricted to the issue of free speech or is the measure you use an instantiation of a larger concept, such as illegitimacy or aggression? You could test this in your dataset by testing the effects of other legitimacy- or aggression-related measures to assess whether suppression of freedom of speech has distinctive effects. At the very least, we ask that you consider this issue in the General Discussion and propose ways this issue might be further pursued in future research.

Related to this point, you should prepare readers in the introduction for the rationale behind this covariate (was it pre-registered?). For all studies, please report the relevant analyses in the SOM and provide a brief summary in the main text of whether the effects hold when not controlling for it, as this factor appears to matter to some extent. In Study 3, for example, you note in a footnote: “Note that the findings remained the same when ANOVA was used (i.e., not controlling for political conservatism) except that the counter-protest group x counter-protest strategy interaction in predicting sympathy for immigrant rights protest did not reach significance ($p = .375$).” This should be moved into the main text and discussed. More generally, we recommend avoiding footnotes.

Reviewer 1, point 4, recommends including a mini meta-analysis “to get a sense of a set of aggregate effects across the various studies.” We suspect that many readers will also appreciate this analysis to highlight the main take-away findings. However, given the variation in methods and few studies per method type in the current work, these findings need to be interpreted very cautiously. Also, the field seems to be quite divided on internal meta-analysis as it is very susceptible to publication bias. So, we ask that you consider Reviewer 1’s point, but if you do pursue it, we ask that you acknowledge the limitations of this approach in the manuscript. If you choose not to follow Reviewer 1’s recommendation, please provide a detailed explanation in your response letter.

Thank you for your transparency regarding deviations from the pre-registrations. Please ensure that these deviations are highlighted in the main text rather than confined to the SOM or footnotes. In addition, make sure to clearly state the original hypotheses for each study and provide corresponding tests, or justify why such tests are not included (the latter option not being optimal). This point is especially important given that some findings are discussed as having been “predicted.” To give another example, Study 3 assessed additional variables specified in the pre-registration. Please explain why these were not included in your analyses, and preferably incorporate them.

In the mediation figures, as one reviewer notes, please clarify in the notes whether the effects are standardized or not, explain what the values in parentheses represent, and report 95% confidence intervals and p -values for all paths. More

generally, whenever you present standard errors, please provide 95% confidence intervals instead. Make sure to explain whether indirect effects described in text are standardized or not.

For all studies, please provide more detail about what participants were told and include clearer descriptions of the experimental materials.

Can you visualize Table 3? This will make findings easier to comprehend than the current table format.

Unless providing direct tests of the null hypothesis, rephrase sentences such as “sympathy towards the White nationalist protest did not shift”. I.e., rather say, “did not shift *statistically significantly*”.

Given the selective dropout in Study 4a, please report whether findings held when the full sample was utilized.

In Study 4b, did you preregister exclusion based on *one or more* failed attention checks? This question links back to the pre-registration point.

In Study 4b, the manipulation check (second Yes/No question) seems to be an attention check. Please clarify. Later you also focus on violence perceptions as manipulation check instead.

Minor Stylistic Issues

There is a typo on line 291.

There is a double word on line 480.

We note that in the text on page 7 line 143, you state, “We conducted four studies across different socio-political contexts” but in the Abstract you indicate, “We conducted five survey studies using correlational and experimental designs.” We understand that a study that has “A” and “B” distinctions might be counted as one or two studies, but you should be consistent in your descriptions.

Please provide two decimals for means and SDs, even when the second digit is a zero. This applies to demographics as well.

Line 671: There seems to be a typo (you likely mean a different measure than “sympathy” here).

For consistency, please either integrate the results and discussions sections for all studies or keep them distinct throughout.

The methods heading is missing for Study 2.

In sum, we see considerable potential in this work, but we do not believe that potential is fully realized in its current form. We therefore invite you to submit a revised version by September 30. If you need additional time, please do not hesitate to contact us. We recognize that the requested revisions may require more space, and we are granting an allowance of up to 2,000 additional words.

Best,

Jonas R. Kunst & John F. Dovidio

Reviewer 1

This manuscript investigates the effects of counter-protests on public support for social change movements across diverse contexts (Hong Kong, Thailand, the U.S., and Australia). Using five survey studies, the authors argue that violent counter-protests often backfire by increasing perceptions of free speech suppression, which in turn fosters greater sympathy for the social change movement being opposed. I read the manuscript with great interest. Focusing on counter-protests as active agents in shaping public opinion is both timely and underexplored. The research has the potential to contribute to the psychological literature on collective action as well as the broader interdisciplinary literature on repression. I also appreciated the researchers' effort to collect data from different national and political contexts. However, I also have several major reservations about the research, which I outline below:

Major issues:

1. **Counter-protests as repression:** The Introduction suggests counter-protests can be viewed as a form of grassroots repression (p. 7). While some counter-protests may indeed aim to silence or be perceived as such, others might express legitimate competing claims in pluralistic democracies. In fact, the very existence of counter-protests can also be interpreted as the opposite of repression - as a sign of a

society's ability to accommodate and debate diverse ideas, which is arguably healthy for democracies. The current manuscript frames counter-protests primarily through the lens of repression, which risks oversimplifying the phenomenon and implicitly problematising counter-protests *per se*. I would encourage the authors to consider this idea more carefully and qualify it, perhaps by limiting it to cases where counter-protests employ coercive or violent tactics (which is also what the data mostly support) or are directly sanctioned or even organised by the government with the goal of silencing.

2. **Research contexts:** The manuscript would benefit from a fuller discussion of research contexts. Most studies were conducted in liberal democracies (Australia, U.S.) where freedom of speech is a strongly endorsed value. Only Study 2 involved Thailand, a non-democratic context, but the implications of this difference are not fully explored. Perceptions of free speech suppression may vary significantly across regimes. Study 2's correlational design further limits what can be concluded about counter-protests in non-democratic contexts. A deeper reflection on these boundary conditions is needed.

3. **Asymmetry in protest framing:** Across all studies, the original protests were described as peaceful or perceived as less violent than the counter-protests, while counter-protests varied in their level of disruption or violence. This structural asymmetry means that the data provide a limited view of the phenomenon: they show that disruptive counter-protests against peaceful, liberal protests can be perceived as suppressing free speech, which in turn increases sympathy for the original protest. However, this is not strong evidence about the effects of counter-protests *per se*, or about violent counter-protests. We do not know how observers would respond if the original protests were also violent or disruptive. It is plausible that the observed effects are driven by the perceived asymmetry between a peaceful movement and a disruptive counter-movement, rather than by counter-protesting *per se*. If the asymmetry were reversed - for example, if a peaceful counter-protest confronted a disruptive or violent original protest - public sympathy might increase for the counter-protest instead. This further limits the generalisability of the findings and presents another boundary condition. In general, I believe the claims and conclusions about counter-protests should always be contextualised and qualified.

4. **Conceptualisation and operationalisation of "violence":** The Introduction describes violent tactics as "involving harm and damage to persons or property" (p. 6). Across the studies, however, violent counter-protests were described to involve yelling, tearing down banners, and taking away microphones. The latter might be viewed as disruptive but not equivalent to physical assault or property destruction. This creates a misalignment between how violence is conceptualised and operationalised. The operationalisation risks conflating disruption with violence. More discussion is needed to clarify whether these tactics should be conceptualised as "violence" or "disruption," and the implications for understanding the findings.

5. **The role of political orientation:** Political alignment with the original movement is arguably one of the most important predictors of perceptions of repression and movement sympathy in these studies. However, participant ideology

is not explicitly reported or discussed until Study 4 – it was controlled for as a covariate rather than additionally reported as a key demographic characteristic of the participants. Earlier studies should have reported participants' ideological orientation more consistently. Because most samples leaned left politically, responses may have been systematically biased in favour of (peaceful) social change movements. While the General Discussion acknowledges this limitation, it is mentioned only briefly and late in the manuscript. This should be reported and discussed more explicitly and consistently throughout.

6. **Framing of the movements:** The Methods sections of Studies 1 and 2 do not provide detailed descriptions of how the movement and counter-protests were presented to participants. This makes it difficult to evaluate whether the framing was neutral or might have influenced participants' perceptions. While I appreciate the publication of study materials on OSF, more details about the text used to describe the protests and counter-protests in the manuscript are needed.

7. **Causality in Studies 1 and 2:** Studies 1 and 2 present mediation analyses as if causal pathways are established, but these are based on cross-sectional correlational data. Alternative explanations seem equally plausible (e.g., people already sympathetic to the protesters may interpret counter-protest tactics as more violent and repressive). Causal language should be avoided with correlational data, and alternative pathways should be acknowledged. Additionally, the Results and Discussion sections of these two studies only present the results without actually discussing them. They could be expanded to engage more critically with the findings.

8. **Analytical decisions.** In Study 3, the mediation analysis omitted the immigrant-rights counter-protest conditions. Since the hypotheses were ideologically symmetrical, I was surprised to read that the conservative protest/liberal counter-protest conditions were omitted altogether. A stronger rationale is needed for the analytical choices.

9. The **General Discussion** focuses heavily on the mediational results. A more critical discussion of the inconsistent direct effects of counter-protest is needed to better interpret and integrate the findings across the experimental studies.

Minor points:

1. The figures do not specify whether coefficients are standardised or unstandardised. Notes should also clearly distinguish direct, indirect, and total effects.

2. Having subscripts in Table 3 to indicate group differences could help the readers navigate the complex ANCOVA results.

3. In Study 3, the reporting of simple effects did not cleanly unpack the interaction effect on sympathy toward the immigrant rights protests. They first compared groups within the violent conditions (violent White nationalist vs. violent immigrant rights counter-protest) and then compared groups within the immigrant rights protest conditions (violent vs. non-violent White nationalist counter-protest). These two

simple effects are not parallel and parallel simple effects should be reported to unpack the interaction effect.

4. On page 31, “*There was a significant effect of condition on sympathy*” should read “*There was a significant effect of condition on perceived suppression of free speech.*”

Reviewer 2

I was invited to evaluate “Understanding the Effect of Counter-Protests on Public Attitudes Towards Social Change Movements.” The paper tackles an important question, namely, how mass publics react to pro-system counter-protests. This is a pressing topic in need of further psychological clarity and this paper meets this need. My evaluation of the paper’s theory and general methodology lead me to recommend a revise and resubmit. In particular, a stronger manuscript will engage (and hopefully resolve) some of the issues I wrestled with when reading the paper.

1) The synthesis of the available literature can be strengthened by drawing on work with an explicit social identity theory focus, as well as extensive research in political science on mobilization and counter-mobilization in protests movements. In terms of the latter point, earlier work by Taeku Lee (2002) is notably missing, but is quite relevant to the researchers’ efforts. In addition, more recent work on more contemporary protests (e.g., anti-police) is rife with tensions, some of which the current paper can actually address (here, please see work by Tyler Reny, Ben Newman, and others).

2) I was not fully clear on the merits of each sample used in the paper. I do like that the paper evaluates its major hypothesis in several contexts, but if we treat each sample as an aggregated unit of analysis, why should readers be comfortable with the spread of these cases? Put differently, I urge the authors to spell out their theory of data, which can help readers better evaluate how strong each set of empirical tests are.

3) I appreciate the convergence of evidence across correlational and (semi-) experimental studies. However, across the collection of studies, the mediator is generally observed, not manipulated, which raises threats to confounding and simultaneity bias. As a user of mediation models, I think the paper can be more convincing for many skeptics of mediation if it employs some type of sensitivity analyses that bounds the indirect effects away from zero (i.e., at what point do the indirect effects vanish if confounding is present). Here, let me suggest the following possibilities:

- a) See work on sensitivity analyses with observational data, including a ShinyApp, by Carlos Cinelli and Chad Hazlett.
- b) See sensitivity analyses for mediation experiments with a measurement-of-mediator design (see work by Kosuke Imai and Tepei Yamamoto)
- c) See work by psychological methodologists, specifically, Georgeson, Alvarez-Bartolo, and MacKinnon (2023) in *Psychological Methods*.

4) Finally, given the trove of data that are reported, it might be useful for readers to get a sense of a set of aggregate effects across the various studies. Here, I think a mini meta-analysis (with random effects) would be super useful and straightforward to implement. See work by Jin Goh (in *Social and Personality Psychology Compass*) and Ramona Alhambra and colleagues (in *Research & Politics*).

Round 2

Editor Decision

Dear Authors,

Thank you for submitting your revised manuscript to our special issue on the *Psychology of Pushback*. We were impressed by the thoroughness of your response and revisions. As the manuscript has significantly improved and you have addressed nearly all of our concerns, we have decided not to send the paper out for another round of reviews.

However, before we can accept the manuscript for publication, we request that you address a set of remaining issues. Most notably, one of these issues pertains to the statistical feedback from the previous review round.

With regards to the comments by the Reviewers in the previous round, we are not satisfied with your response regarding the feasibility of sensitivity analyses, as the claim that your regression-based approach is incompatible with the reviewer's suggestions is methodologically incorrect. For standard linear mediation models with continuous mediators and outcomes, which appears to be the design used in your study, the Ordinary Least Squares (OLS) estimation used by Hayes' PROCESS macro and the Potential Outcomes framework used by Imai and colleagues yield mathematically identical point estimates for the indirect effect. Therefore, there is no theoretical inconsistency or need for model respecification to utilize these tools; the mediation package in R simply offers a computational layer for sensitivity diagnostics that PROCESS currently lacks, but it operates on the same underlying statistical relationships in linear settings.

You do not need to abandon your original regression results to satisfy this request, but you must validate them. Because the Average Causal Mediation Effect (ACME) calculated under the Potential Outcomes framework converges with the product-of-coefficients method in linear settings, you can run the standard mediate function in R solely for the purpose of generating the input object required for the medsens function. This would allow you to report the rho parameter (the correlation between error terms required to zero out the effect) without altering your primary reporting of the PROCESS coefficients. Alternatively, the method proposed by Cinelli and Hazlett using the sensemakr package is expressly designed for OLS regression and is

entirely compatible with the second equation of your mediation model (the regression of the outcome on the mediator and treatment).

We ask that you conduct a sensitivity analysis using either of these established methods to bound your indirect effects away from zero. The reviewer is correctly identifying that the sequential ignorability assumption is a significant threat to validity when the mediator is not manipulated, and dismissing this concern based on software limitations is not an acceptable rebuttal. Please perform this analysis and report the results, to demonstrate that your findings are robust to a reasonable degree of unobserved confounding.

In addition, we noted a few minor issues that need to be addressed.

- **Causal Language:** Thank you for your efforts to avoid language that may suggest causality. Please ensure you also avoid the use of "direct" or "indirect effect" when describing correlational data (especially in the first two studies). Instead, please use terminology reflecting the associational nature of the analyses.
- **Missing Statistics:** On line 801, test statistics are missing (they are currently only presented for the first comparison).
- **APA Formatting:** Please write out numbers under 12 in words, in line with APA 7 standards, unless they refer to statistics.
- **Technical Check:** Please upload your document to <https://apa.advances.in> for a format check. There are a few minor but important issues related to URLs (specifically blank spaces that corrupt functionality) as well as missing DOIs. Please also run the manuscript through the DOI check. Note that the check is probability-based, so you may ignore false positives.

In sum, we believe that your paper has become much stronger and is close to reaching the level we envision for contribution to this special issue. Given the minor nature of changes, we request the revised version by December 20. If you need additional time, please do not hesitate to contact us.

Best,

Jonas R. Kunst & John F. Dovidio